admission either from Lehmann or from me. Lehmann wrote in his original paper: "Ein exacter Beweis hierfür (*i. e.*, for his explanation) kann wohl im Augenblicke nicht geführt werden." Nor, I take it, in any future Augenblick.

On the other hand, I have never regarded this point as the point at issue. Lehmann set out to examine telepathy at large. He chose the Sidgwick experiments simply as typical series, considering the authors' names a guarantee of serious intent and careful work. In his inquiry he laid hold of a condition which had never been thoroughly investigated before, and traced its effects in experiments that were both ingeniously devised and rigidly controlled; no one can neglect the unconscious whisper in future telepathic work. His paper is a model of scientific method; he has shown us how borderland questions are to be attacked, and proved that the 'ordinary channels of sense' have unexplored resources. His suggestions will be fruitful, for the next stage of advance must be an exhaustive study of the 'number habits' which Sidgwick at first rejected, but now makes the headstone of the corner. Even granting all the contentions of the critics, therefore, I should assert that Lehmann's work is brilliant, and that it has done signal service to scientific psychology. But, as I hinted before, I do not know that quasi-mathematics has contributed much to psychology in any field of research.

I conclude with a word on the logic of Professor James' objection. A theory is propounded which, from the outset, lays claim to probability and to probability only. 'Exact proof' is acknowledged to be impossible. Criticism plays upon the theory, and the author again acknowledges that his hypothesis is not proven. Professor James, apparently forgetting the first acknowledgment, affirms that the criticism has 'exploded' the theory! What is not proven is, *eo ipso*, exploded! Is Professor James, then, ready to grant that his recent book on 'Human Immortality'—something which assuredly is not yet proven—is an 'exploded document'? If the alternatives before me are scientific isolation and companionship on these logical terms I prefer the isolation. E. B. TITCHENER.

NOTES ON PHYSICS.

THE COMPENSATION PYRHELIOMETER.

MOST of the measurements heretofore made upon radiant energy by means of the thermopile or bolometer are relative rather than absolute in character, and the necessity for a simple and accurate method for reducing the indications of such instruments to the usual thermal units has long been felt. On this account a paper by Knut Ångström (*Wied. Ann.*, No. 3, Band 67) in which he describes an instrument for measuring radiation in absolute units is of great interest. This instrument, to which he has given the name of Compensation Pyrheliometer, is apparently simple in construction, and the results obtained from it are very reliable, the maximum error, as the author states, not exceeding 2%.

The construction of the instrument is briefly as follows: Two equal, thin (.001 to .002 mm.), blackened strips of platinum are mounted in such a manner that either or both, by means of appropriate shutters, can be exposed to the radiation to be measured.

One of the two junctions of a small constantin-copper thermo couple is attached to each of the rear surfaces of the platinum strips, the circuit of thermo couple including a galvanometer. It is evident that if one of the platinum strips is exposed to radiation the equality of temperature at the junctions is destroyed and the galvanometer is deflected. A current of electricity is now made to traverse the unexposed strip, and the strength of the current is adjusted until the galvanometer returns to zero. Under these conditions the two junctions are receiving the same amount of energy per second, and the heat developed by the current in the unexposed strip is equal to that given to the exposed strip by the radiation. A knowledge of the strength of the current and of the resistance of the strip suffices to find the value of the radiation in gramme calories per square centimeter per second. Since the strips are alike in all respects and are subjected to identical conditions, no corrections are necessary.

An interesting result obtained by Ångström is the value of the mean horizontal radiation of a Hefner normal lamp, which comes out to be 13.2 gm.-cals. per square centimeter per minute

at one centimeter distance. This value seems to be very constant, and the Hefner lamp may possibly become a standard of *total* as well as of *luminous* radiation.

A. St.C. D.

NOTES ON INORGANIC CHEMISTRY.

Two papers have appeared in the *Journal of the American Chemical Society*, by Dr. F. P. Venable, on the 'Nature of Valence.' The idea of valence in chemistry has been of gradual growth and has merely been the expression of certain chemical facts. In the case of the carbon compounds and in organic chemistry in the hands of Kekulé it has proved of immense service, and without it the wonderful development of this field in the past three decades would have been impossible. Its application to inorganic chemistry has been hardly as happy, and the original conception of a fixed valence has been abandoned for that of variable valence, but even this is limited to comparatively simple compounds. As an explanation of the structure of double salts, water of crystallization, metal-ammonia bases and other complex inorganic compounds it is wholly inadequate and possibly a hindrance. While in one form or another the conception of valence has permeated and, one might almost say, dominated chemistry, little or nothing has been known regarding its nature. To be sure, in the last decade or so several hypotheses have been offered by van't Hoff, Wislicenus, Victor Meyer, Knorr, Flawitzky and a few others, attributing valence to electrical phenomena, space relations of the atom, etc., but none of these attempted explanations has received any measure of support. The hypothesis which Dr. Venable puts forth is that valence is dependent upon vibratory (or kinetic) equilibrium of the atoms. "The question as to whether the atoms of two elements will unite is decided by affinity which is in some way connected with the electrical condition of the atoms. There is no apparent connection between this and valence." But the atoms "are endowed with motion, and this motion probably varies in velocity and phases with the different elements." "A molecule, in order to exist, must maintain a certain equilibrium and harmony between these various motions, so that there can be all degrees of equilibrium from the very stable to that which may be upset by the least disturbing influence from without." Variable valence will be, in part at least, dependent upon the temperature, and a "sufficiently high temperature may prevent any harmony of motion whatever being attained, and hence union may become impossible." Valence would then be dependent upon the possible harmony of motion between the different atoms. The hypothesis is simple and satisfactorily explains many at least of the facts; thus, for instance, the zero valence of elements like argon and helium might be due, not to their possessing no chemical affinity (though this may be the case), but to their motion not being capable of harmonizing with that of any other element. The weak point of the hypothesis is the difficulty of proving it to be true. It would be necessary to first know the nature of the motion of the atom, a problem yet unsolved. It is possible that the spectroscope could aid, but at present we have no clue as to why some elements, as iron, furnish a complex spectrum, while others, like sodium, give a relatively simple one. At all events Dr. Venable's idea furnishes a good and simple working hypothesis, and one which may have its practical uses for teachers.

Attention should be called to the *First Supplement* to Dr. H. Carrington Bolton's *Select Bibliography of Chemistry*, 1492–1892, which has just been published by the Smithsonian Institution. It includes works omitted in that volume, and brings the literature of chemistry down from 1892 to the close of 1897. Dr. Bolton has been fortunate in having the cooperation of a number of scholars abroad, who have contributed more than 2,000 titles in Arabic, Finnish, Japanese, Bohemian, Dutch, Portuguese, Swedish, Danish, Norwegian and Russian, no less than 760 titles in the latter language being furnished by Professor A. Krupsky, of St. Petersburg. Dr. Bolton's bibliographical work is invaluable to chemists and is carried out in a manner which is above criticism.

Professor F. Emich, of Graz, has been kind enough to send me a paper from his laboratory by F. Dörner, with a chemical investigation of the cement from antique water conduits. The